weekly periodical, devoted to the 'gesammtgebiete der naturwissenschaften.' The first numbers are mostly filled with abstracts and reviews.

— The London *Daily telegraph* states that an effort is at last being made to disinter the Sphinx. The work of exhumation is intrusted to Brugsch Bey, brother of the distinguished archeologist, who will carry out a plan formed by Signor Maspero. About 20,000 cubic metres of sand must be cleared away. To expedite this task a little tramway has been constructed, and 150 laborers are engaged for the more mechanical portion of the toil. About Easter the work is expected to be completed. Then, when the rock out of which the statue has been hewn is laid bare, a broad circular walk will be constructed around it, and a high wall built to guard against future encroachments of desert sands.

— A correspondent of the New York *Herald* says that it is very probable that Mr. Rousseau, who was sent by the French government to inspect the Panama canal, must report that the present enterprise is inevitably to be changed from a sea-level canal to a canal with locks, if it is ever to be finished by the present company, thereby not merely falsifying M. de Lesseps's assurances a hundred times reiterated, but also the very basis of the preference given to the Panama route over that of Nicaragua. Regular subscriptions to the funds are exhausted, and it is proposed to raise a hundred or more million dollars by a national lottery.

— It is expected that the Grecian canal, connecting the gulfs of Corinth and Aegina, will be completed by the end of the present year. The canal will be less than three miles in length, but the deepest cuttings are nearly two hundred and fifty feet in depth. The canal will admit the passage of the largest ships, and will shorten the sea distance between the Adriatic and the levant a hundred and thirty miles.

— In a recent paper the eminent French *savant*, Alphonse de Candolle, reproduces with approving comments the arguments of Prof. A. Graham Bell upon the production of a race of deaf-mutes in the United States. In commenting upon the methods proposed to prevent this result, he adds that the English language is the least favorable of all for spoken use among deaf-mutes, as the movements of the lips are more often replaced by an accentuation or intonation that does not produce any visible effect. The vowels are articulated less clearly than, and are not so sharply differentiated from each other as, in the other chief European languages. The French has very few words, such as *de* and *crac*, in which the lips do not take part in the pronunciation, while in English numerous sounds, as of *n*, *th*, and *h*, are formed almost wholly by the action of the tongue. This is confirmed by the experience of intelligent deaf-mutes. Mr. Candolle suggests, in addition to the views of Professor Bell, that, independently of deaf-mutism, marriage between first-cousins should be wholly prohibited. He also asks whether greater care given to new-born infants would not materially diminish the number of deaf persons.

— A new edition of 'Berghaus' physikalischer atlas' is announced, to be completed in twenty-five *lieferungen*, the first of which will appear about the middle of the present month. The work is prepared wholly anew, by the co-operation of Drs. Drude, Gerland, Hann, Hartlaub, Neumayer, and Zittel.

— The bird-destroying 'slung-shot' boy is not an eastern innovation. A writer in the Santa Barbara, Cal., *Press* deplores the evil that he has grown to be in the west, in the destruction of the native birds for millinery purposes.

— The following works are announced by the Smithsonian institution to be now in press: 'Scientific writings of Joseph Henry;' 'Flora of North America,' by Asa Gray; 'Guesde collections of antiquities,' by O. T. Mason; 'Annual report for 1884;' 'Paleontological bibliographies,' by J. B. Marcou; 'Bulletin of the Washington philosophical society,' vol. vii., for 1885; and the different reports of progress in 1885: viz., in chemistry, by H. C. Bolton; in geography, by J. K. Goodrich; in seismology and vulcanology, by C. G. Rockwood.

LETTERS TO THE EDITOR.

Oil on troubled waters.

ONE of the most curious things in connection with the use of oil on troubled waters is the frequency with which it appears as a new discovery. Those who would dismiss the subject with a contemptuous sneer at the credulity of people imposed upon by sailors' yarns know little of the prolonged attention the matter has received in the past, and of the honored scientific men who have studied the problem. There is no room here to quote the many observations at hand, but only to sum them up, and to present the explanation that has met with most favor.

The earliest reference at hand in English is found in Cavallo's 'Philosophy' (fourth American edition, 1879, p. 209). The author points out that oil spreads 'instantly' over water; that the wind has little effect in raising waves on the surface of oil, or of water covered with a film of oil; and that from early times this fact has been utilized in stilling the waves of the sea. The experiments of Franklin and others are cited.

In Gehler's 'Physikalisches wörterbuch' (Berlin,

1837, band vi. p. 1750 ff.) there is a good presentation of the subject, and many facts are cited. Most of them are drawn, however, from the thirty-page discussion in Weber's 'Wellenlehre' (1825). Here one finds quotations from Aristotle, Plutarch, and Pliny, which show that in early times the power of oil to quiet, and so render more transparent, the surface of water, was known. References are made to other and later writers, and to the facts collected by Franklin; and details are given of experiments made by him (Phil. trans., lxiv., 1774), and later by the Webers.

From all these data, as well as from the recent observations reported in *Science* (especially vii. p. 134), it seems that the effect of the oil-film is to diminish the 'combing' of the waves, and to prevent, in part at least, the formation of small waves, and the growth and sharpening of the crests of the large ones by the continued action of the wind. The exaggerated popular notion that the great waves are quieted seems to be erroneous. The only known ways of destroying in the open sea the energy of a wave once formed are by fluid friction, by rain, and by an opposing wind. But we must not underestimate the advantage of preventing the piling-up, on a wave already dangerously high, of another only a few inches high. On the well-known principle of superposition, it must sometimes happen that the crests of waves belonging to two or more systems will coincide. The resultant wave is then higher, and exposes more surface to the wind; and the crest, being sharper, is more easily blown off by the wind: so, as the wave is likely to run faster than the ship, it may break over her in a way that would not happen if it were only a little lower,— if only one of its smaller components could be suppressed.

It is further to be borne in mind, in seeking an explanation for the indisputable and useful effect of oil, that, as the passage of a wave is the transfer of energy but not of matter, the oil will not be carried onward by the wave; and that, if the formation of new waves over a given large surface could be prevented, the old ones would speedily pass out of it, and those coming into this surface from beyond would not be increased, but would decrease somewhat, because of the fluid friction.

The practical problem, therefore, before the shipmaster, is to find some means, 1°, of preventing the formation of new waves, or the growth of old ones, over a given surface to the windward of his ship, and, 2°, of making this surface as large as possible. He solves it more or less completely by the use of oil; and now we seek an explanation of the action of oil from the physicist.

The German physicists of the first part of this century followed pretty generally the view attributed originally to Aristotle, and elaborated by Franklin; the Webers subscribe to it; and Müncke, in 'Gehler,' says it is generally held: in a word, the friction of the wind is less on the oil than on the water. Stated in this way, however, the sentence is almost sure to convey a false impression. We know of absolutely no proof that this is true, if taken with its obvious meaning; but the truth it embodies is simply, that, owing to the interposition of the oil-film, the force of the wind is not communicated to the water; and this can be explained in a way consistent with modern physical notions. Franklin had pointed out how a ripple raised by the wind gets higher, broader, and longer at each successive vibration [and therefore travels faster]: he compares the effect of the wind to the setting of a heavy church-bell to swinging, by properly timed impulses of a finger. He thinks the adhesion between the oil and water is so slight (if, indeed, the repulsion be not strong enough to maintain the film at a small distance from the water) that the film can be moved a little by the wind without disturbing the water. He suggests, further, that the wind can 'catch' hold of the large wave better when this is covered with ripples, while, if it be oiled, the wind may press it down. The Webers add something with reference to the resolution of the force of the wind, which seems not quite sound in theory; and Müncke has something to say about a slight binding of the surface of the water by the oil.

But some properties of fluids unknown to the earlier physicists have a bearing on the present problem. Thus Daniell, in his 'Principles of physics' (p. 247), says, under the title 'Superficial viscosity,' "To the same cause [superficial tenacity] we must attribute the smoothing of the surface of a rough sea when oil is poured upon it: the new surface has great superficial tenacity and small superficial tension, and is not readily broken up into surf." The bearing of this may be shown thus. Imagine a perfectly calm lake: a wind strikes it, and it is covered with wavelets. It is not the increase of pressure over the lake that causes the waves, but slight differences of pressure between neighboring points, due to the fact that the winds flow more or less in gusts, not steadily. If the surface were solid, or very viscous, like mucilage or thick oil, the momentary force due to the difference of pressure would cease to act before any sensible movement could take place. The effect would be the same in kind, though differing in amount, however thin the film, or slightly viscous the oil may be; but we should remember that the superficial viscosity which is effective here is usually greater than the viscosity calculated from experiments where a considerable volume of the liquid is used. The effect, too, would be the same in kind, though the sea were rough instead of calm. We see, then, that an oil-film, by its viscosity (as well as by slipping over the water, if Franklin's view is correct), delays the action of the wind's force on the water for so long a time, that the force may have ceased to act before any movement begins, and then no work is done by the wind on the water. Thus, in an extreme case, no new waves are formed, and those driven on by the wind through the oil-covered surface do not have their crests continually elevated and sharpened till they are ready to break.

What might happen in an extreme case does happen, to some extent, in every case where oil is used on the water. Thus the wake of a ship generally shows a surface covered with bubbles more persistent than usual, and comparatively free from small waves, both effects being probably due to the traces of oil coming from bilge-water, the cook's galley, etc. Where a ship is driven before the wind, and the waves are running faster than the ship, if oil is being used, it is evident that the wind has to pass over a long oil-covered surface, and the effect of the oil will be especially favorable. Since it is essential to this explanation that the oil be spread to the windward, little benefit is to be expected from the use of oil on waves coming from a distant storm; nor when the wind is ahead, unless means can be used to throw the oil a long distance ahead.

If this explanation be correct, as we believe it to

be, there is no violation of the fundamental law of modern physics,—no destruction of energy.

The second practical problem is to cover as large a surface as possible with the viscous fluid. Fortunately this can be done easily in accordance with principles explained in many modern treatises on capillarity: for the surface tension of the film between water and air is so much greater than the sum of the tensions, oil-water and oil-air, that a drop of oil is very rapidly drawn out over an enormous surface. If this paper were not already so long, some numerical data might be given. The preference shown for animal or vegetable oils over mineral oils (*Science*, vii. 133) is probably justified by the smaller surface tension and greater viscosity of the former; though it may be noted, that, the greater the viscosity, the slower the oil will spread, other things remaining the same.

To render complete the explanation of this interesting and at first sight puzzling action of oil, experiments are needed by physicists in the laboratory, where for various oils the several physical properties above named shall be measured, and also experiments and observations at sea when wind and waves are moderate enough to be measured, and the captain may go in any desired direction without danger. A few days' observations, where the conditions can be controlled, would be worth hundreds of the desultory reports which the hydrographic office is wisely collecting.

CHARLES K. WEAD.

Professor Thorell and the American Silurian scorpion.

Professor Thorell, who is perhaps the best authority upon the Scorpionidae, both recent and fossil, has rather severely taken to task some of my statements and determinations in connection with the recently discovered American Silurian scorpion (see *American naturalist* for March, 1886, p. 269). In fact, so sharp and pungent are some of his remarks, that a person reading them would naturally infer, that, in Professor Thorell's opinion, I was hardly capable of making a reliable observation, at least not upon a scorpion. He has shown his good nature, however, in the outstart, by admitting that the specimen is really a scorpion, and not a Eurypteroid, — a conclusion the exact contrary of that jumped to by one critic upon reading the first announcement of its discovery. For this concession Professor Thorell has my heartiest thanks. In his further criticisms, however, he is much less lenient, and I wish to briefly notice his objections in their order.

After making the above-mentioned admission, Professor Thorell proceeds to deal with the six ventral plates of this, what he calls, 'rather badly preserved fossil.' In my description in the American museum bulletin, I mention that the specimen is 'greatly compressed;' that the 'dorsal crust is preserved over about two-thirds of the surface,' mentioning the parts; and that "over the rest of the prae-abdomen and what remains of the post-abdomen or tail, parts of the first five segments, the inside of the ventral crust is exposed." This feature of the specimen has, I fear, misled Professor Thorell, and caused him to fall into an error, into which, if he had known the nature of the preservation of the fossils (Eurypteroids) found in the formations from which the scorpion was obtained, he probably would not have fallen. The specimen is greatly compressed vertically, as are all the fossils in the same rock. Along the left side of the abdomen there is a line of fracture, to the right of which the substance of the dorsal plates, and the filling between them, to the ventral plates below, has been removed in splitting the rock, and probably left on the other part. Along this line the thickness between the two sides of the fossil (dorsal and ventral) is about a twentieth of an inch or less. In speaking of this feature, Professor Thorell says, "The whole upper side of the abdomen is broken or cracked longitudinally," and that the articulations of the ventral parts are "all direct continuations of the articulations between the dorsals." Neither of these assertions is entirely true. The abdomen is partially removed, but not 'cracked' in the sense in which he uses the term; and the articulations between the joints of the ventral plates are not 'direct continuations' of those of the dorsal. Besides this, the overlapping of the plates show directly which is dorsal, and which is ventral; and no zoölogist would be apt to make the mistake. If we examine the abdomen of a beetle, roach, or scorpion, on the exterior, we find the anterior plates all overlapping those behind, both dorsally and ventrally: but, if we take off the crust and examine the inside, we find the reverse to be the case; that is, the anterior edge of the plates overlaps the one anterior to it. Now, this is precisely what is seen on this specimen: on the left side the anterior plates overlap those behind, while on the right side the posterior overlap those in front; and the surface of the plates is concave, while on the left side they are convex; so that a mistake is nearly impossible. Professor Thorell's statement, that, if his interpretation of this character is the right one, "the want of spiracles on the plates needs no further explanation," is therefore of no value, as he reasons from false premises: all his conclusions based upon his assumed features fall to the ground, and the want of spiracles is yet unexplained. There are six of these ventral plates plainly seen, extending from beneath the dorsals. Neither is the specimen a 'rather badly preserved fossil,' but instead an exceedingly well preserved and distinct one, as far as the parts existed when the specimen was embedded.

In a footnote to his observations on the above structure, Professor Thorell states, that, "even if the plates in question really were ventral plates, the first (or sixth when counted from behind forward) would seem, from its position, to correspond to the anterior half of the first ventral in the ordinary scorpions, and not to the small plate situated between the pectoral combs." On this statement I will make no comment, further than to say that I have failed to find, in the living species which I have examined, any case where the first (or anterior) ventral plate is even apparently articulated to the third ventral plate, or has the lateral width of this one.

Professor Thorell next goes on to say that "Mr. Whitfield thinks, that, whereas modern scorpions carry the tail (post-abdomen) arched upward over the back, Proscorpius, and also Palaeophonus, carried it in the opposite way, or curved downward." He says, "This would indeed be a character of fundamental importance for distinguishing the Silurion scorpions from all other members of the group," but that to him it is "impossible to find any stringent reason for adopting this strange hypothesis," and that it would cause "the animal's gait to be exceedingly difficult and awkward if it were to walk